Do what you Love — In Conversation with David Mumford

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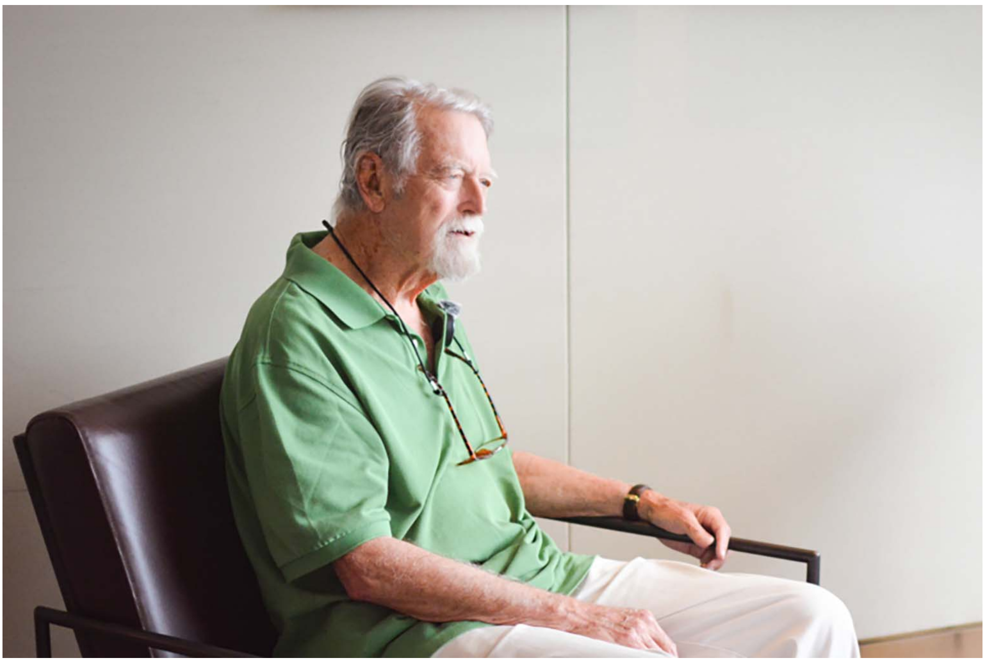


Biographical Sketch. David Bryant Mumford is an American mathematician best known for his work in the field of algebraic geometry and for research into vision and pattern theory. Born on June 11th, 1937, in Worth, Sussex, United Kingdom, Mumford made profound contributions to mathematics and left an indelible mark on the discipline, earning him numerous awards and prizes including the *Fields Medal* (1974), the *Shaw Prize* (2006), and the first *Basic Science Lifetime Award* (2023) in Mathematics. He has also been elected to the National Academy of Sciences, the American Academy of Arts and Sciences, and the Royal Society.

Mumford pursued his graduate studies at Harvard University, earning

a Ph.D. in mathematics in 1961 under the supervision of Oscar Zariski. His dissertation entitled *Existence of the moduli scheme for curves of any genus* paved the way for his future groundbreaking work in algebraic geometry and particularly Geometric Invariant Theory.
Throughout his career, Mumford held various academic positions, including faculty appointments at Harvard University and Brown University, where his passion for teaching and research inspired generations of mathematicians and scientists. He also served as the President of the International Mathematical Union from 1995 to 1999.
The interview is taking place in the hotel suite of Prof. S.T. Yau in front of the picturesque scenery of Yanqi Lake. For the first part of it, Prof. Yau is quietly sitting at his desk in the same room.

SH: *First of all, we would like to congratulate you very much on being the first recipient of the Basic Science Lifetime Award.*

DM: Thank you.

SH: *Of course, this is not your first big award. How do you feel about it?*

DM: I have several thoughts. One is, like with all the other awards that I've received, it always seemed to me that there were at least three times as many people who could equally well have gotten this and other similar awards over the years. I think somehow there's an element of luck and timing, and somehow, moduli problems became a rather sexy subject. And as such, I became more prominent than many of my colleagues who were doing equally deep and important work in other fields. So I always felt a little bit awkward, and I know so many people, good friends, who've done fantastic work and have not been as richly rewarded as I have.

LH: *What did you feel when you received the email about the award? Did you know before that there's something going on?*

DM: Yeah, I sensed that there was something going on. I mean, I have been an old friend of Professor Yau's for many decades. In fact I first met him... I'm not sure... I have a sense that it was UCLA but it might have been Berkeley, but certainly somewhere in California.

[**Prof. Yau shouting from his desk: UCLA.**]

DM: What? So, UCLA. I gave a lecture about what was happening about surfaces of general type. And he comes up to me afterwards and says: 'By the way, I can prove that the inequality you mentioned is a simple corollary of my proof of the existence of Einstein–Kähler metrics.' I said: 'WHAT? WHAT? You can prove that?' He had this air, that he has today also, of total confidence. Of course, I totally believed him, and I was excited since it was a real revolution in the area to prove the existence of these metrics.

So, anyway, when I first heard that Yau was putting together this conference, it seemed amazing, as China had been so closed during COVID. And suddenly he's talking about an annual series of conferences, which will be a major event in three different fields.

But you've got to hand it to Professor Yau, he thinks big and he's incredibly successful. And I just had lunch with him today. I was congratulating him on putting this conference together. I mean it would seem pretty clear that there were various obstacles because it was not announced until extremely late. I mean most major international meetings are announced a year in advance, or something like that. When it comes to getting, for instance, a Chinese visa that's not entirely simple.

SH: *You have been to China several times before and even won some prizes here.*

DM: I've been three times, actually. I was in Hong Kong during the handover[1] and then came to Beijing to talk to the Chinese Mathematical Society about the facilities for the 2002 International Congress. I was at that time, President of the IMU and so I was here to check out whether or not the facilities were going to be adequate. And at that time—you're too young to remember this—it was bicycles everywhere. It was hardly a car on the street anywhere, but the streets were jam packed with bicycles.

And I thought to myself, okay, so 5 years from now, you can rent bicycles, I'm sure mathematicians would love it to rent bicycles. What happens is that 5 years from then, suddenly the city was jam packed with cars.

LH: *You can rent bicycles now.*

DM: It's an evolution that has been a wonder to watch. And just in the short time I've been here, yet again, everything seems to have totally changed. It's just phenomenal.

KS: *As a former President of IMU, what do you think of the aim of ICBS of having math, physics, and CS together?*

DM: I think it's marvellous, but it also requires a lot of lectures. I think it is not been the tradition to teach your graduate students in mathematics, how to give a really good lecture, which is broadly accessible. And there is certainly an unfortunate tradition of giving a completely technical lecture, which probably only a dozen people in the world could follow.

And I think that the idea of bringing these fields together will only be successful, if somehow there is a certain pressure on the invitees to really think more about how they can make as much of their talk, at least half of their talk, really, broadly accessible by giving background information and links to the more distant areas. Anyway, it's EXAMPLES, giving concrete examples whenever possible.

KS: *I hear you had a hard time writing an obituary of Grothendieck for the magazine* Nature *[7] explaining his work.*

DM: The whole idea was that I couldn't say complex numbers. I don't know, I just guess I was always aware there was a gap, but it was certainly bigger than I had expected...

[1] The handover of Hong Kong from the United Kingdom to the People's Republic of China was at midnight on 1 July 1997.

KS: *I learned you wrote a paper[2] on the prediction of eclipse motion coming from ancient China. How did you get interested in this topic?*

DM: Astronomy was always a secondary subject that I kept up with to a certain extent. That decade when I wrote that paper, I got intensely involved with the history of mathematics. I was not aware of how tremendously hard it is to predict eclipses. In the article, I have the front page of the New York Times sometime around the 1920s in which there was a major total eclipse, visible in half of Manhattan. The headline of the New York Times was 'ECLIPSE FOUR SECONDS LATE HERE.' Even in the 1920s! It's a three-body problem. Still, the predictions were off by 4 seconds. So, I got really quite intrigued by this business.

LH: *How good were these predictions in ancient China?*

DM: Yeah, that's the most curious. This is again a complicated story. Professor Qu Anjing was a collaborator. He visited Harvard and we had a seminar on comparing eclipse predictions. We were analyzing the Yuan dynasty calendar for how it handled parallax.

When you see the moon directly overhead, you're seeing it on a straight line from the center of the earth to wherever it is. But suppose it's near the horizon. Then you're seeing the moon sideways. That means you're 4000 miles off of the straight line that joins the moon to the center of the earth. If you look at that, there's a triangle. And the moon's position, depending upon where it is, relative to the horizon, varies by as much as 1 degree. The size of the moon is half a degree in diameter. So, that will totally shift you from an eclipse to a non-eclipse. So it is hopeless to predict eclipses with any accuracy at all unless you have some knowledge of this parallax, the lunar parallax.

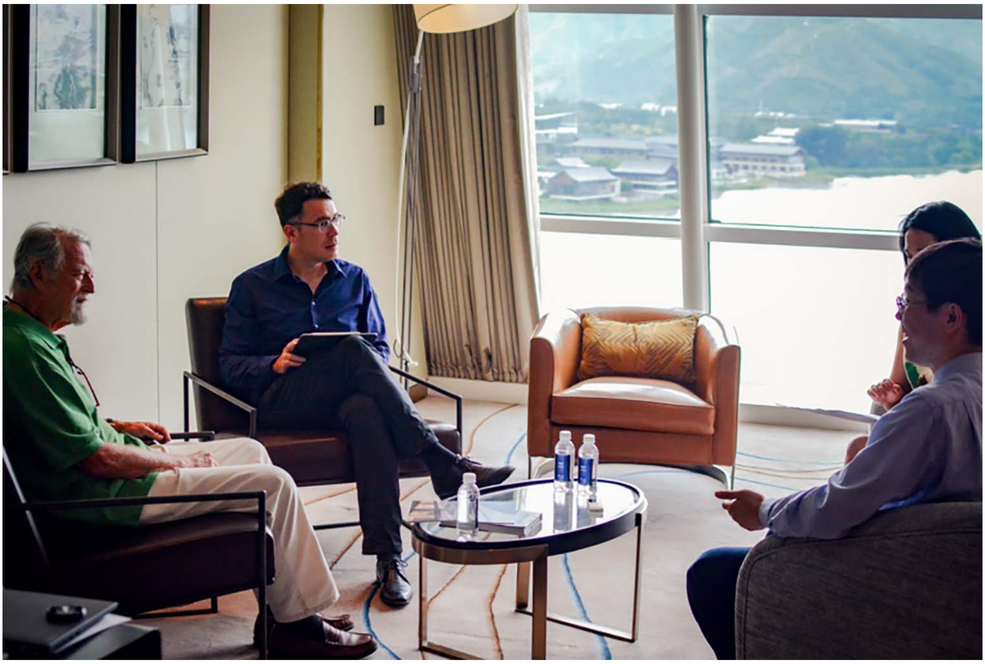
It is still an open question to what degree Chinese people knew this. You require a geometric model in which the earth is round and you have to know how big it actually is. The Yuan dynasty calendar has a really weird, semi-accurate way to approximate this parallax correction. Professor Qu was analyzing this in our seminar.

The story really begins in the Tang dynasty. I'm convinced that the famous monk Yi Xing knew clearly the size of the earth and knew about parallax. But it was never written down! So how did that happen? This has been my venture more deeply into Chinese history. I find it absolutely fascinating.

SH: *Yes it is, thank you. Now let's return to modern times. You are the first recipient of the Basic Science Lifetime Award for your fundamental contributions to algebraic geometry, and in particular, to moduli theory. Can you briefly introduce us and the general reader to this topic?*

DM: For the general reader I've always felt that the best way to talk about moduli is to talk about maps. I mean what is a map? It is a piece of paper, usually, or can be a computer screen. It's a flat, small chart, points of which correspond to actual points somewhere on the earth. You're making a single diagram that has its

[2] [5]



points in one-to-one correspondence with the points on some part of the Earth or maybe the whole Earth for that matter. This idea of summarizing a collection of things, which itself can't really be visualized very well in something more concise, which you can see and examine, that is the concept of a map.

We want to make a map whose points are defined by surfaces like spheres or donut shapes or pretzels. The number of "handles" on them is called their genus, 0,1 or 3 in the examples. And I know you guys know what I'm talking about, but I'm just trying to phrase it in the way you said. The set of these surfaces with their conformal structure is a mathematical thing, which you can define very easily. But the question is, can we sort of treat it as an object itself? So that's what a map is. You need to make a map of all the surfaces of genus g. That's the moduli space. That's the way I've explained it previously to non-mathematical audiences.

SH: *How did you choose moduli spaces as a research topic?*

DM: I had always been drawn to maps. I remember drawing a map of the neighborhood where I grew up, and drawing a map when I was visiting my aunt and cousin in England, a map of their neighborhood.

I originally fell in love with algebraic geometry through the idea of infinitely near points and blowing up, so birational geometry. That was the original hook that made me think 'WOW, this is some weird universe': algebraic varieties with all sorts of interesting properties. But then once I got into the field, I mean the fact that the way they were constructing it (moduli spaces) was only known through this roundabout complex analytic method. And that only gave you the thing in characteristic zero. Anyway, it seemed like a nice problem.

KS: *Do you have any personal stories about your two approaches to the moduli spaces, geometric invariant theory and so-called Deligne-Mumford stacks?*

DM: It was very funny—the situation with Deligne—because literally, our letters crossed. There was no Internet in those days. I had been playing around with how to compactify the moduli space of curves and do the whole moduli space over $\mathbb{Z}$ and he, independently, had been playing around with it, too. And we came up with essentially the identical proof. But it's also a little scary. Fortunately, both of us are gentlemen. We didn't dispute[3]. But it was really weird.

The same thing happened with my thesis [6], actually. These coincidences are not totally rare. One of the corollaries of developing geometric invariant theory was constructing a moduli space for vector bundles over curves. And weirdly, halfway around the world[4], there were Seshadri and Narasimhan, looking at unitary representations of the fundamental group and what vector bundles these define. And bingo! They came up with the same definition of the restriction that you have to make in order to get a moduli space. I mean, it was really weird.

LH: *It was before the Internet. So, how did you find out that someone else did the same?*

DM: So in this case, I actually published a short note in the Bulletin of the AMS. They had written a paper, but I don't think it was yet published, but anyway, I got this letter in the mail, from India with all sorts of exotic stamps on it. It changed my life completely because Seshadri became one of my very closest friends. Sadly, he's passed away now, but I must have visited India at least a dozen times.

KS: *How about the story with Deligne?*

DM: About the Deligne story? I was a little shaky about whether the foundations of stacks were sufficiently well developed that we could be confident about applying all sorts of theorems to them. But fortunately, he is so systematic. He really is, when he does something, he covers every base. He wrote that section of the paper on the background data about stacks. I mean, the stacks were in the air. Everybody knew that stacks were the appropriate objects. And later, another, very old friend of mine, Michael Artin, proved this phenomenal existence theorem for stacks. That sort of nailed it, that stacks were, without a doubt, the most natural context in which you should really place algebraic geometry. Anyway, so that worked out well. But Deligne and I have just a certain intersection at that point in our lives.

SH: *When you started your work in the moduli, you have been in Harvard, right? As you already mentioned, stacks were around. So, how important was it for you to be at the right place?*

DM: I would say it was hugely important that I was at Harvard. The biggest reason was that Grothendieck visited twice for a semester and the opportunity to listen to his lectures and talk to him personally was huge.

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[3] Instead, Deligne and Mumford wrote the joint paper [3].

[4] At TATA Institute, Mumbai, India

Because of Zariski, everyone came and visited Harvard at some point; Serre came and talked about Grothendieck–Riemann–Roch theorem that had just been published. And so I think one way I was very lucky was that the whole cohomology of coherent sheaves and everything you could do with it was in the air. And I think I was one of the main people who really pushed all sorts of ways to use cohomology of coherent sheaves.

I will tell you a funny anecdote. The Tata Institute in Bombay has periodic international math conferences. I forgot how frequently they are, but these conferences are in January, where they invite all sorts of foreigners. I went there for a conference in algebraic geometry, and André Weil went there too. Bombay was officially a dry city at that time. But as a foreigner, you could sign a piece of paper saying that it was medically necessary for you to ingest a certain amount of alcohol every month. And you got what was called a permit card. It had a certain number of units of alcohol. And when you went to these permit stores, they would calculate these crazy fractions. They didn't use decimals for some reason. The crazy fractions as to how many units you are now being charged with against your monthly allowance. Anyway, this is a preamble.

We gave a cocktail party in our apartment and invited all the speakers in the conference. So there I was a little bit tipsy and perhaps Weil was too. And so he asked me: 'What are you lecturing on?' I said: 'I'm lecturing on abelian varieties.' And I said: 'Well, you know, your book *Variétés abéliennes et courbes algébriques*, I don't like your book.' I have decided it's my marching order to completely eliminate the use of Jacobians—because he really proved almost everything by using the fact that there was a Jacobian—and to use cohomology instead.

I think that appealed to him that I was brash enough to say that. I mean he was not an easily approachable guy, André Weil. In some sense, Weil and Zariski really invented modern algebraic geometry. And then Grothendieck capped it all off. But Weil essentially had no graduate students. He was impossible to work with.

If you give a lecture in Princeton and he attended it, it was known that he would always make some cutting remark at the end of this lecture. When he didn't do that, when my lecture ended, I felt greatly honoured. Instead, what he does is he invites me to come to his office the next morning, because I had submitted a paper to the Annals of Math about the construction of the moduli space. I sit down in his office, and he looks at me and he says: 'Where did you go to school?' And I think he's so fatherly. He's asking me about my career. And I said: 'I went to Phillips Exeter Academy, and I went to Harvard...' 'No, it wasn't that,' then he said, 'I wondered because your paper is written in a style usually used only in correspondence between close friends and is completely unacceptable for a journal.'

[**Laughter**]

Then and there, I said I will never publish a paper in the Annals of Math. I didn't say it out loud. I vowed it.

That's so scary. I don't know whether you want to print this.

Prof. Yau: That statement is quite well-known to the community.

DM: [To Yau] I didn't know you were there listening to me.

KS: *How about Zariski? I hear you had a small card to remind him of nilpotent elements in a ring. Any other personal stories with Zariski?*

DM: So this is a story that I've told before. [To Yau] I'm getting more cautious.

Prof. Yau: Well, I will get inside. [leaves the interview room]

DM: So he had the habit that he would go to Nantucket[5] every summer, rent a house. Nantucket has become a very fancy upper-class resort now, but at that time, it wasn't, in any sense. It was a very down-to-earth, pleasant way to really get away from everything. And he had a little sailboat that he would sail in the harbor of Nantucket, which is quite large. And so he would come back typically with some manuscripts with stuff he'd written over the summer. And he would then have a seminar with his students. So at one point, he comes back with this manuscript.

It was this monograph of his on minimal models that he had just completed. We had this seminar and he decided he's gonna let his students speak. So mostly this was me, Hironaka and Michael Artin. I don't know if there's a 4th person, somehow the feeling that might have been a 4th. Anyway, he would have us lecture him about the material, but then came the kicker. He would not announce who he would call on. You had to be prepared every single time to stand up and give a talk. This was terrifying.

SH: *Maybe in the end, it was one of the seminars you learned the most.*

DM: Maybe. So, anyway, he pointed to me. He wanted me to lecture on Bertini's theorem. So I say: 'For simplicity, let's treat the characteristic zero case.' And he says: 'No, the whole point is to do it in characteristic p!' Yeah, that was something.

SH: *Moduli spaces are still a very dominant topic in many areas of mathematics, like algebraic geometry, differential geometry and number theory. But also string theory and theoretical physics. So do you follow some of the recent developments in these areas?*

DM: Well, you clearly haven't looked very closely at my CV. I have actually written papers about a differential geometric moduli spaces.

KS: *In the 2000s.*

DM: In the 2000s, yeah. And I've been collaborating with Peter Michor, who has a wonderful approach to infinite dimensional Riemannian geometry. I mean it's based on looking at maps of curves into the space and defining structures on this infinite dimensional space by pulling them back by an arbitrary C^∞ map of some finite-dimensional object into this space.

Anyway, so again, moduli spaces played a role. He had all the nasty details of dealing with infinite-dimensional space down cold. And I could just pretend everything that held in the finite-dimensional case held in this case, too. And he could write the paper in such a way that it was. In any case, I got then

[5] Small island in the Atlantic ocean belonging to Massachusetts.

very intrigued with one particular moduli space since we're dealing with infinite dimensions. From my point of view, what you want to realize is that Cartesian space, in infinite dimensions, from my perspective, is just the whole ladder of Sobolev spaces and anything in between. Similarly, when you have a moduli space, you're gonna have a ladder of them, depending on in which topology you want to make it complete. In any case, my very favorite space of all has been the space of simply closed plane curves, which is also known as the infinite-dimensional universal Teichmüller space, which has many other beautiful metrics on it with completely different completions.

Anyway, to answer, I'm aware that these other moduli spaces have made a huge impact. I remember Drinfeld's shtukas. But on the whole, I have not followed that. Except that, I do have a paper [4] where we began to investigate what I think is reasonably called the Chow manifold of a finite-dimensional Riemannian space. It's the collection of finite dimensional submanifolds. You can restrict it topologically, if you wish, and get something which has only one component, for instance.

Another moduli space is related to waves. There's the most incredible metric on the space of simple closed plane curves. I mean many of these versions of this space, with a Sobolev metric on them, have a very weak cotangent space, but with a fairly strong tangent space. For instance, it can have delta functions in its cotangent space, which then give you a soliton-like behavior. But this metric is the opposite. It's a very weak structure on the tangent space, but very strong structure on the cotangent space. It was really invented by a Russian fluid dynamics guy named Zakharov[6]. That's been my favorite, recent moduli space.

This problem arises because there's a theory of rogue waves, which is still fairly primitive. I don't think the phenomenon of rogue waves is well understood. The fluid mechanics people that have studied actual water waves make an ansatz, which seems to me is throwing the baby out with the bath water. But this leads to theta functions and theories that do produce rogue waves. But it's still not a rigorous theory.

KS: *How do you generally find your research questions? Do you have any strategy?*

DM: It's what I fall in love with.

KS: *Are you more like an example-based person?*

DM: Yeah, I like to have some concrete examples to start with. I mean the reason I was able to understand what stochastic processes is was by first studying Brownian motion. I mean everything is there really in the theory of Brownian motion. And then it becomes much easier to generalize. So now I like to have concrete examples.

SH: *What are the research questions you are interested in at the moment?*

DM: One of them: I have always been intrigued by entropy. I want to understand... You see, Schrödinger proposed that photosynthesis was the fundamental process which throws out a great deal of low-grade heat and produces some

[6] Vladimir Evgenevich Zakharov, born 1939, passed away in August 2023 after the interview took place.

macroscopic order. But I've never read a good treatment of, a really careful thermodynamic treatment of photosynthesis. And it seems to me perfectly possible. But I don't know.

Actually, I have been, in the last year, very preoccupied with a forthcoming book, which the American Math Society is putting out, which is entitled 'Numbers and the World: Essays on Math and Beyond.'[7] The worst part of it was permissions. I mean when you write a blog, you just take anything anywhere off the Internet and use it. The Internet is the wild west. Copyright doesn't really exist on the Internet. But once you want to publish something as a book, unfortunately really have to care about copyright. Once I was forced to omit a clipping from a paper because of copyright. The New York Times wanted a bloody fortune in order for the Japanese Mathematical Society to publish my article about eclipse prediction. I wanted to have the front page of the New York Times that I mentioned earlier in this interview. I was unable to reproduce it because the [NY] Times required so much for copyright.

SH: *So do we have five more minutes? Let me ask one question about artificial intelligence because everybody is talking about it at the moment, and it has quite a bit impact on our lives...*[8]

DM: You see, I've actually studied at some depth, the neurophysiology of mammalian brains and I have written quite a few papers on this. And it's my strong feeling that right now someone has to take the step of not merely training language models, but giving whatever program eyes and ears and mobility in the actual real world. I mean why do these language models hallucinate all the time? It's because they have no actual experience of the real world. Everything is just a story as far as they're concerned. And therefore, they don't know truth from fiction and so on. I feel that once you begin putting together different modules like adding something for interpreting images to language models, you have new problems. You begin to need to introduce much more architecture. I mean GPT, the whole GPT sequence, has an almost trivial architecture with an encoding half, and then a decoding half. The human brain, all mammalian brains are very modular and have components that are linked together through the white matter. And they're gonna have to introduce more architecture into the successive programs. Now, I don't think it's gonna be that hard. Actually, I'm a believer that fully humanoid robots are going to be manufactured within 50 years.

LH: *Is it a scary thought?*

DM: Of course, it's scary, but there are so many scary things. I mean being able to manipulate our genes using CRISPR is hugely scary. It's going to make eugenics possible. I think the human race will speciate. I mean, global warming is scary. There are so many scary things.

LH: *Is it more fascinating than scary or more scary than fascinating?*

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[7] This book has recently become available for purchase in the AMS Bookstore.

[8] For another take on AI see the interview with Michael Jordan in this issue.

Figure 1. From left to right: K. Shimizu, D. Mumford, S. Heller, L. Heller, H. Du.

DM: I think it's fascinating. I guess for me, it's more fascinating. Look, I'm 86.

KS: *Let's finish with a positive remark. Do you have any career advice for younger scientists?*

DM: Do what you love!

SH: *So maybe the very last question, some suggestion for a very good article or book, every scientist, or everybody should read.*

DM: I actually love the books by Nick Lane. He's written books about the biochemistry [2] and the evolution of life on the Earth [1]. They are relatively easy reading, but at the same time, they're really accurate. I love his books.

LH&SH&KS: *Thank you so much for your time and your stories.*

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